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Pressed to Prolong: Conscription, the Costs of Military Labor, and Civil War Duration

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Existing research has identified numerous explanations for why some civil wars last longer than others. Yet, the type of labor that state militaries recruit has remained unexplored in this context. We consider how a state's military personnel system affects its *ex post* decision to keep fighting. We argue that conscription renders access to military labor relatively easy and, thus, less expensive. As military wages fall, war becomes less costly, the production of military power becomes more labor intensive, and the hazard of conflict termination declines. In a volunteer force, in contrast, military labor is relatively scarce and, therefore, more expensive. Accordingly, war becomes more costly, the production of military power becomes more capital intensive, and the hazard of conflict termination rises. These effects are reinforced as a conflict persists, leading to an increased divergence in duration across conscripted and volunteer militaries. We test these contentions using a global sample of civil wars, finding robust support for each expectation. We also validate the underlying mechanisms linking conscription to protracted conflict in two illustrative cases. Our results highlight the importance of labor-side determinants of war duration and contribute to a growing literature that explores how the composition of military forces affects conflict dynamics.

A través de la investigación actual se identificaron numerosas explicaciones de por qué algunas guerras civiles duran más que otras. Sin embargo, el tipo de mano de obra que reclutan los ejércitos estatales no se ha estudiado en este contexto. Consideramos cómo el sistema de personal militar de un estado afecta su decisión posterior de seguir luchando. Sostenemos que el reclutamiento permite acceder a la mano de obra militar con relativa facilidad y, por lo tanto, con menor costo. Conforme disminuyen los salarios militares, menos costosa se vuelve la guerra, la producción de poder militar se vuelve más intensiva en mano de obra y disminuye el riesgo de finalización del conflicto. En cambio, el personal militar de un ejército voluntario es relativamente escaso y, por lo tanto, más caro. En consecuencia, la guerra se vuelve más costosa, la producción de poder militar se vuelve más intensiva en capital, y el riesgo de finalización del conflicto aumenta. Estos efectos se refuerzan a medida que persiste un conflicto, lo que lleva a una mayor divergencia en la duración entre los ejércitos reclutados y los voluntarios. Comprobamos estas afirmaciones con una muestra global de guerras civiles, constatando un fuerte consenso para cada expectativa. También validamos los mecanismos subyacentes que vinculan el reclutamiento con la prolongación del conflicto en dos casos ilustrativos. Nuestros resultados ponen de manifiesto la importancia de los determinantes laborales en la duración de la guerra y contribuyen a la creciente bibliografía en la que se explora cómo la composición de las fuerzas militares afecta a la dinámica del conflicto.

Des recherches existantes ont identifié de nombreuses explications des raisons pour lesquelles certaines guerres civiles duraient plus longtemps que d'autres. Pourtant, le type d'effectifs recruté par les armées d'État reste inétudié dans ce contexte. Nous examinons la manière dont le système de gestion des effectifs d'une armée d'État affecte sa décision a posteriori de continuer le combat. Nous soutenons que la conscription rend l'accès à des effectifs militaires relativement simple et donc moins coûteux. Comme les salaires militaires baissent, la guerre devient moins coûteuse, la production de puissance militaire implique davantage d'effectifs et le risque de fin de conflit diminue. À l'inverse, dans une force de volontaires, les effectifs militaires sont relativement plus rares et donc plus coûteux. Par conséquent, la guerre devient plus coûteuse, la production de puissance militaire exige davantage de capital et le risque de fin du conflit augmente. Ces effets se renforcent au fil de la persistance du conflit, ce qui mène à une augmentation de l'écart entre la durée d'un conflit pour les armées de conscrits et la durée d'un conflit pour les armées de volontaires. Nous avons mis ces affirmations à l'épreuve en nous basant sur un échantillon mondial de guerres civiles et nous avons constaté un solide soutien à chacune de nos hypothèses. Nous validons également les mécanismes sous-jacents associant la conscription à une prolongation des conflits par deux cas illustratifs. Nos résultats mettent en évidence l'importance des déterminants côté effectifs pour la guerre des guerres et contribuent à une littérature croissante qui explore la manière dont la composition des forces militaires affecte les dynamiques des conflits.

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Introduction

Research on civil war duration has identified numerous explanations for why some conflicts last longer than others, including the capacity of rebel groups to fight (e.g., Cunningham, Gleditsch, and Salehyan 2009), geographic distances and dependencies (e.g., Buhaug, Gates, and Lujala 2009), external intervention (e.g., Anderson 2019), technologies of rebellion (e.g., Balcells and Kalyvas 2014), and bargaining failures (e.g., Walter 2009). Surprisingly, however, practically no attention has been given to a factor that we believe—and show—is key to explaining why some civil wars last longer than others: a state's military recruitment strategy.

This inattention is notable given the breadth of extant findings concerning the roles of (1) state military recruitment strategies in interstate conflict processes (e.g., Choi and James 2003; Horowitz, Simpson, and Stam 2011; Pickering 2011) and (2) technologies of war in shaping the conduct of armed conflict more generally (e.g., Biddle 2004; Kalyvas and Balcells 2010; Caverley and Sechser 2017). The former body of research has found that state military recruitment strategies significantly influence the outbreak and consequences of interstate war, whereas the latter research program has highlighted the ways in which mechanization, strategy, and relative strength each shape and constrain combatants' use of force. Yet, the potential significance of a state's military personnel system has been overlooked in the intrastate conflict literature. This article addresses this limitation, illustrating the importance of taking different personnel systems into account in civil conflict research and policymaking.

We examine how military personnel systems affect states' *ex post* decisions to continue fighting a civil war. Adopting a political economy approach, we explore how variation in the labor costs associated with conscript versus volunteer militaries impact conflict duration. Conscription—a technology that allows militaries to retain a constant pool of entry-level recruits—renders military labor relatively abundant and, thus, less expensive. Holding other costs constant, as military wages decrease, war becomes less costly, the production of military power becomes more labor intensive, and the hazard of conflict termination therefore declines. In contrast, in a volunteer military—that is, a force that must attract the same individuals by offering market-competitive wages—military labor is relatively scarce and, thus, more expensive. As military wages increase, war becomes more costly, the production of military power becomes more capital intensive, and the hazard of conflict termination rises. Moreover, we explain why these costs should be reinforced as the war drags on, leading to a greater divergence in conflict duration across volunteer and conscripted militaries.

From these insights, we derive two expectations: (1) states with conscript militaries are likely to fight longer civil wars relative to states with volunteer militaries, all else equal, and (2) the hazard of conflict termination in states with conscripted militaries decreases over time compared with countries with volunteer militaries. In testing this argument empirically on a global sample of civil wars fought between 1945 and 2003, we find robust support for both expectations. We also validate the underlying mechanisms linking conscription to protracted conflict in two illustrative cases: the Colombian and Nicaraguan civil wars.

By exploring the effect of military personnel systems on conflict duration, this article demonstrates the importance of accounting for labor-side determinants in civil war research. We elaborate upon the implications of our results for both researchers and policymakers in the conclusion.

Military Personnel Systems and Civil War Duration

Past Research on Recruitment and War

Contemporary scholarship on military personnel systems emerged in the midst of the Vietnam War and heightened domestic confrontations over the draft in the United States. Early work focused on the economic trade-offs associated with conscript versus volunteer systems (e.g., Altman and Fechter 1967; Hansen and Weisbrod 1967; Fisher 1969). More recent work has broadened the analysis to include other determinants of states' decisions to conscript their citizens, such as domestic institutional arrangements (e.g., Levi 1996), colonial legacies (e.g., Asal, Conrad, and Toronto 2017), and external threat environments (e.g., Cohn and Toronto 2017).

Notwithstanding the breadth of this literature, existing scholarship has centered primarily on questions related to *inter*-state conflict. This research has produced a number of important findings on military recruitment strategies and the employment of force in the international system. For example, conscript armies have been found to be more likely to be involved in militarized interstate disputes (Choi and James 2003). They are also more likely to initiate hostile military operations against interstate opponents (Pickering 2011). And whether as a war initiator or the target of aggression, conscript militaries suffer significantly more casualties in interstate wars (Horowitz, Simpson, and Stam 2011).

Much less is known about the significance of military personnel systems for the durations and dynamics of *intra*-state conflicts. To date, most existing studies have examined the recruitment strategies of rebel organizations (Gates 2002; Weinstein 2006; Eck 2014), or the determinants of civilian participation in armed conflict (Wood 2003; Kalyvas and Kocher 2007; Humphreys and Weinstein 2008), with less consideration of the military labor policies of states afflicted by civil war (for an exception, see Chaudhry, Karim, and Scroggs 2021). Yet, there are reasons to believe that a state's approach to military recruitment affects its behavior during internal conflict. For example, states with conscript militaries have been found to be more likely to initiate combat operations against rebels relative to states with volunteer militaries (Pickering 2011). Variation in state recruitment strategies has also been linked to variation in state-perpetrated sexual violence (Cohen 2013) and civilian killings (Koren 2014).

In this study, we contribute to the existing literature by examining the impact of military personnel systems on an outcome of interest that has, to date, gone unexamined: civil war duration. We argue that relying on conscripted forces increases the duration of internal war—an effect that grows larger as a conflict endures. To explain why, we develop a theoretical argument centered on variation in the labor costs associated with conscript and volunteer militaries.

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Military Personnel Systems and the Costs of Military Labor

A state's ability to meet its military objectives rests on its capacity to recruit and retain capable individuals to its military services. Under volunteer personnel systems, states rely on the voluntary services of professional soldiers to staff their military ranks. Recruits willingly sign up for service in exchange for compensation and other benefits provided by the state. Because they rely on willing volunteers, states with volunteer militaries must compete with the wider labor market to recruit and retain soldiers. High-quality recruits have alternative employment options in the civilian sector and in many contexts they can also choose to attend college. Volunteer militaries must therefore offer a compensation package that is at least as good as (if not better than) the value of their services elsewhere in the economy. In this sense, military labor attracts its true market value under a volunteer military personnel system.

The need to compete for and attract labor entails a variety of costs for volunteer militaries. For example, volunteer militaries often rely on enlistment incentives—such as signing bonuses, scholarships and tuition assistance, life insurance programs, and post-service benefits—to attract combat personnel (Miles 2006). They also provide more competitive compensation packages, including higher basic pay rates, more comprehensive benefit packages, and more modernized living quarters. Proficiency pay is especially important for attracting and retaining individuals with sought-after technical expertise or special skills, such as doctors, dentists, and optometrists (US General Accounting Office 1978, 23–24). There are also additional organizational costs associated with a volunteer force. For example, volunteer militaries must invest additional resources in recruiting activities to seek out qualified candidates. This includes hiring military recruiters, advertising available positions, and "selling" the military services as a vocation. In short, for states with volunteer militaries, "[w]ell trained volunteers are valuable and scarce" (Horowitz, Simpson, and Stam 2011, 913).[1]

The US government's decision to transition from a conscript to a volunteer force is instructive. The change added an average $1.93 billion to the military's annual budget in 1970 dollars (or roughly $13 billion per year in 2020 dollars). The majority of these annual cost increases were attributed to "additional compensation paid to junior grade service personnel" (US General Accounting Office 1978, ii, 3). In fact, basic pay nearly doubled in real terms for less experienced service members between 1971 (2 years before the end of the draft) and 1975 (2 years after it ended) (Congressional Budget Office 2007, viii, 12). Tellingly, these increases brought military pay roughly into line with average pay for full-time, high-school graduates in the civilian sector (Cooper 1977, 42).

In contrast, states that employ conscription rely on the extracted services of mandatorily recruited soldiers. All individuals within a designated group must register for military service and may be "called up" at the state's discretion; a failure to comply constitutes a punishable offense. As a compulsory form of recruitment, conscription enables states to bypass the wider labor market when staffing positions in their armed forces. Since they do not need to compete for labor, states with conscription can pay less than the prevailing market wage for their military personnel. Stated otherwise, the price of military labor deviates from its true market value under conscription.

The difference in a conscript's military wage and the wage they would otherwise receive in the wider labor market amounts to a "conscription tax" (Duindam 1999, chapter 4). This concept captures the additional economic burden carried by conscripts, rather than the polity as a whole, in the production of military power.[2] In effect, conscription transforms the observable "taxes-in-money" levy that is imposed on the general public into a hidden "taxes-in-kind" levy that is imposed on conscripts alone. This is one reason that conscription policies can be difficult to change: for most members of the general public, conscription is functionally equivalent to a tax benefit (it reduces the economic burden they must otherwise bear to fund their military). A second reason these policies persist is their effect on a state's costs of military power: conscription reduces the budgetary costs of war.

Budgetary costs refer to the actual money outlay required to fund a war effort. This captures direct financial expenditures on military labor and capital, but excludes indirect costs, such as losses in productivity elsewhere in the economy. Economists have shown that the *real* (or social) costs of conscription can exceed its *budgetary* costs over the long term for a range of force sizes. Depending on the demand for military labor, the size of the "conscription tax," the costs associated with preventing evasion from service, recruitment and training costs, and the dead-weight tax loss associated with taxation to pay for military labor, conscription can be inefficient relative to an all-volunteer force (Warner and Negrusa 2006). The important distinction between budgetary and real costs notwithstanding, however, we focus our discussion on the budgetary costs of war for conscript versus volunteer personnel systems for three reasons.

First, we seek to understand how variation in financial outlays associated with raising military labor affect *ex post* decisions to continue fighting an ongoing civil war. While the real costs of conscription can exceed its budgetary costs over the long term, the opportunity costs and economic distortions it introduces only become observable with time (Poutvaara and Wagener 2007, 9, 12). For this reason, the short-term budgetary costs associated with specific military operations, rather than the long-term inefficiencies associated with different military personnel systems, will drive political decision-making in states confronting immediate threats from ongoing insurgencies.

Second, despite its potentially greater real costs, conscription can be politically expedient for state leaders confronting rebellion. As Duindam (1999, 59) explains, "[c]onscription is a tax, but cannot be found in our national ledgers." Because conscription is an invisible "taxes-in-kind" levy imposed on only a subsection of the population, it enables governments to avoid visible "taxes-in-money" increases on society at large. This is advantageous in the context of an ongoing battle against insurgents for the civilian population's "hearts and minds," during which time governments will seek to avoid fiscal tax increases. Here again, the short-term budgetary costs of war will dominate political decision-making, rather than the long-term inefficiencies associated with conscript personnel systems.

Finally, our focus on budgetary costs follows existing work on the political economy of military power (e.g., Cappella Zielinski 2016). A broader conceptualization of costs—one that takes into account economic inefficiencies, opportunity

[1] For a broader discussion of the interplay between the market and militaries, see Levy (2010).

[2] The "conscription tax" concept can also be broadened to include other costs imposed on conscripts, such as career disruptions. For example, earnings of drafted US veterans of the Vietnam War were roughly 15 percent less relative to comparable nonveterans, an earnings reduction equivalent to the loss of 2 years of civilian labor market experience (Angrist 1990).

costs, and the benefits of comparative advantage—is critical for debates over the long-term economic, social, and normative implications of different military personnel systems. However, in this article, our interest lies in understanding how the fiscal outlays necessary for conducting military operations affect decisions to continue fighting a civil war. Consequently, in what follows, we focus our discussion on the budgetary costs associated with raising military labor in the context of an ongoing conflict.

The Costs of Military Labor and Civil War Duration

The number of battle-related fatalities imposed by a conflict increases as a function of its duration (Lacina 2006). Daily death rates likewise rise as armed conflict endures (Lujala 2009). As body bags accumulate, so too do the costs of war. Notwithstanding their horrific consequences for combatants and civilians alike, however, the effect of deaths and injuries on a state's costs of military labor varies as a function of the military personnel system it employs. We argue that these contrasting cost effects are central to understanding the link between military personnel systems and civil war duration by way of two mechanisms.

The first mechanism linking military personnel systems to civil war duration is the *bargaining range effect* induced by variation in labor costs. Consider a standard bargaining model of war, in which warfare is conceived as a costly learning process that reveals information about combatants' relative capabilities and resolve (e.g., Wagner 2000; Filson and Werner 2002; Werner and Yuen 2005). These models demonstrate that wars end when combatants' expectations about the likely outcome of future rounds of fighting converge and a bargaining range emerges based on those common expectations.

The bargaining range, which encompasses the set of agreements that both sides prefer to continued fighting, is a function of the combatants' expectations about the likely outcome of the war, less the costs of war to both sides (Fearon 1995, 387). Holding all else equal, factors that decrease the costs of war shrink the size of the bargaining range, thereby increasing the likelihood of continued bargaining failure. In effect, decreasing war costs encourage combatants to dig in their heels and drive a harder bargain. This delays the convergence of expectations, as combatants postpone negotiated settlement in the hopes of greater concessions in the future. In contrast, factors that increase the costs of war expand the bargaining range, thereby decreasing the likelihood of continued bargaining failure. As the costs of war increase, the price combatants must pay to hold out for a better deal rises. This accelerates the convergence of expectations, as combatants are encouraged to adopt more conciliatory bargaining behaviors.

These insights provide inferential purchase on the ways in which different military personnel systems affect bargaining dynamics in protracted civil wars. In particular, we argue that variation in the military labor costs associated with voluntary versus conscript systems affects the size of the bargaining range. In doing so, they affect the likelihood of continued bargaining failure and the hazard of civil war termination.

First, consider a voluntary military personnel system. In a volunteer military, soldiers are scarce and expensive. Longer wars entail increasing numbers of casualties, which further decreases the available supply of military labor. Holding demand constant, a decrease in the supply of labor will increase personnel costs, given the state's need to attract and retain talent from the wider labor market with more competitive compensation packages. This increases the budgetary costs of a civil war. As a state's budgetary costs of war increase, the set of agreements that it prefers to war expands.[3] This accelerates the convergence of expectations among the domestic combatants, as the state begins to adopt more compromising bargaining positions. In turn, the likelihood of conflict termination increases.

Conscript militaries, in contrast, face much weaker labor constraints. Longer conflicts still entail an increasing number of casualties, but without a concomitant reduction in the available supply of military labor. New conscripts are simply compelled into service; they are not in a position to demand better compensation. Consequently, a state's budgetary costs of war increase to a lesser extent, and the set of agreements that it is willing to accept over war expands more slowly.[4] States with conscript militaries will therefore be more willing to delay negotiated settlement, opting to fight longer civil wars to extract greater concessions from their rebel opponent. Stated otherwise, conscription decreases the hazard of conflict termination.

The second mechanism linking military personnel systems to civil war duration is the *capital–labor substitution effect* induced by variation in labor costs. The production of combat power requires both labor and capital inputs, where "labor" refers to military personnel and "capital" refers to both physical capital (e.g., protective gear, armor, and aircraft) and human capital (e.g., rigorous training, military education, and new skills development). Changes in the wage (labor) to rental (capital) rate ratio lead to reallocations of the factors of production of combat power and to corresponding changes in the composition of a state's military forces (Duindam 1999, chapter 5). When the wage to rental rate ratio increases, states substitute away from the relatively more expensive input (labor) and into the relatively cheaper input (capital). In contrast, when the wage to rental rate ratio declines, states substitute out of the relatively more expensive input (capital) in favor of the relatively cheaper input (labor).

Because military labor is rendered artificially inexpensive by way of compulsory recruitment, the production of combat power is likely to become more labor, and less capital, intensive in conscript militaries. In practice, this means that states with conscription will under-invest in physical and human capital—they will rely on larger numbers of less well-equipped and less effectively trained soldiers relative to states with volunteer militaries, which will rely on smaller numbers of better-equipped and better-trained recruits. Poutvaara and Wagener (2007, 7) highlight the "often lamented tedium of service, the over-manning of army units, and the excessive maintenance devoted to weapons and material in conscript armies" that reflect distortions in capital–labor ratios. Former US Secretary of the Navy Richard Danzig (1982, 110) puts the point more bluntly: "[w]hen it receives people at no cost, the military, like most institutions when this happens, tends to treat them as if they were virtually of no worth [...] Tales of military misuse of quality conscripts are probably as old as the military itself."

[3] This proposition is in line with Levy's conception of the (volunteer-based) "market army," for which "militarism is subjected to the market" and "economic calculations about security govern military activity." See Levy (2010, 380).

[4] We qualify that the budgetary costs of war increase *to a lesser extent* because there will always be costs associated with casualties, such as the need to pay compensation to family members of the deceased or to provide medical services to the injured. Our point, however, is that military wages are unlikely to be affected. Consequently, states with conscript militaries will see lower war cost increases relative to states with volunteer militaries, all else equal.

Studies that have examined transitions from conscript to volunteer systems confirm the capital–labor substitution mechanism at work. Bove and Cavatorta (2012, 285), for example, find that increasing labor costs associated with transitions to volunteer personnel systems result in "a greater emphasis on equipment and infrastructure and less reliance on soldiers." Conscript militaries also have higher rates of force turnover owing to higher initial attrition and lower reenlistment rates. The "depreciation" of human capital is therefore greater in conscript systems, further skewing capital–labor ratios. This is consequential in light of the complexity of modern conventional military operations and counterinsurgency campaigns—both of which demand high levels of skill and training (Biddle 2004; Corum 2006). Rather than teach their recruits advanced tactics or how to use more sophisticated weapon systems, "an organization staffed with draftees is likely to use less advanced technology" (Poutvaara and Wagener 2007, 7).

We argue that the capital–labor ratio underlying a state's production of military power has important implications for a civil war's duration. While ground mechanization or air power in isolation is insufficient for the prosecution of a rapid counterinsurgency campaign (Lyall and Wilson 2009; Kocher, Pepinsky, and Kalyvas 2011), militaries capable of employing combined arms tactics—that is, tactics that employ a mix of mechanized infantry, armor, and aircraft—fight shorter civil wars, on average. As Caverley and Sechser (2017, 707–708) explain, combined arms doctrines facilitate decisive early engagements in three ways. First, combined arms militaries enjoy superior mobility and logistics, undermining rebels' abilities to draw out conflicts in remote areas. Second, combined arms strategies help prevent rebel dispersion and retreat—guerrilla tactics that prolong civil wars. Third, given their intense organizational demands, combined arms operations more quickly reveal a military's capabilities and capacity to execute strategy. Taken together, these effects accelerate the convergence of combatants' expectations about the likely outcome of future battles, thereby increasing the likelihood of civil war settlements. Notably, Caverley and Sechser (2017, 713, 717) show that this is true for both conventional civil wars *and* insurgencies.

These results dovetail with the broader literature on the advantages of combined arms strategies. Bennett and Stam (1996), for example, find that maneuver strategies produce shorter interstate wars. Horowitz and Reiter (2001) and Allen and Martinez Machain (2019) show that air power is more successful when deployed alongside ground forces. And Martinez Machain (2015) demonstrates that the combination of air and ground forces decreases the duration of air campaigns.

Crucially, combined arms doctrines are capital intensive: they require significant investments in physical capital (e.g., armored personnel carriers, self-propelled artillery, and air platforms) and human capital (e.g., skills development, organizational expertise, and training) (Talmadge 2015). Conscript militaries, which are more labor intensive in their production of military power, are therefore less likely to deploy combined arms strategies relative to volunteer militaries. By extension, they are more likely to fight longer civil wars. As Caverley and Sechser (2017, 708) put it, "[l]ess-demanding strategies—attempting to overwhelm the enemy with sheer manpower, for example—take longer to achieve their intended objectives." In contrast, volunteer militaries, which are more capital intensive in their production of military power, are more likely to employ combined arms strategies. Accordingly, they are likely to fight shorter civil wars.

A corollary of both the *bargaining range effect* and *capital–labor substitution effect* mechanisms is the expectation that the duration effects associated with different military personnel systems grow over time. To see this, consider that labor replacement costs linearly increase for volunteer militaries: each additional recruit hired to take the place of a war casualty pushes the state further up the wage scale, given the need to compete with the wider labor market. As battlefield casualties grow over time, and as the risks associated with military service increase, recruitment and retention become more challenging (Korb and Duggan 2007, 468). Finding replacements for those injured or killed on the battlefield therefore necessitates further increasing compensation packages, as the state must return to the labor market and attract new volunteers that were previously uninterested in military service. In this sense, replacements entail ever-increasing payroll costs. This magnifies both the bargaining range effect and the capital–labor substitution effect, leading to an ever-increasing likelihood of conflict termination (relative to conscript militaries) the longer a conflict endures.

In contrast, labor replacement costs are constant for conscript militaries: each additional recruit hired to take the place of a war casualty is paid the same wage as all other individuals. New personnel are brought onto the force via compulsion; there is no need to compete with the wider labor market and there is no need to raise wages to recruit and retain each additional soldier. Even as battlefield casualties accumulate and the risks associated with military service grow, wages can remain flat. Holding force size constant, conscript militaries' payroll costs are unaffected by casualties. This serves to ameliorate both the bargaining range effect and the capital–labor substitution effect, resulting in an ever-decreasing likelihood of conflict termination (relative to volunteer militaries) the longer a conflict endures.

These arguments produce a set of testable hypotheses about the relationship between states' military personnel systems and civil war duration. First, owing to both the bargaining range effect and the capital–labor substitution effect, we expect that conscription is associated with longer civil wars relative to volunteer military personnel systems. This is a generalized expectation: it should hold across the wide range of contexts in which civil wars are fought around the globe. Accordingly, this expectation is formalized as:

- H1: State military conscription will decrease the hazard of civil war termination relative to civil wars fought by state militaries employing volunteer forces.

Second, we expect that the duration effects imparted by military personnel systems will increase in their intensity *as a function of time.* In particular, we anticipate that the war-prolonging effects of conscription grow larger as a conflict endures. This expectation is formalized as:

- H2: The civil war prolonging effects of state military conscription will rise over time as a civil war persists.

Empirical Analysis

Sample and Dependent Variables

To evaluate the above hypotheses, we begin with Cunningham, Gleditsch, and Salehyan's (2009) 1945–2003 sample of civil war durations.[5] This sample, and Cunningham, Gleditsch, and Salehyan's analysis thereof, represents

[5]We specifically use the corrected version of these data, made available by Cunningham et al. (2012).

the most comprehensive evaluation of disaggregated (i.e., dyadic) rebel and state fighting capabilities in the context of civil war duration to date. The data accordingly retain only active civil conflicts and are structured into time-varying government–insurgent (i.e., conflict-dyad) observations for any violent conflict that generated at least twenty-five fatalities in a given year. These durations are recorded in days until the final day of a given calendar month. In keeping with Cunningham, Gleditsch, and Salehyan (2009), civil conflicts that exhibit peace spells of 2 or more years are treated as separate conflict dyads. We henceforth label this outcome variable as *civil war duration*.

Estimation Models

Consistent with Cunningham, Gleditsch, and Salehyan (2009) and others (e.g., Bagozzi 2016), we use semi-parametric Cox proportional hazard models with standard errors clustered on conflict-dyad id to estimate the effects of our covariates on a dyad's *civil war duration*. Our Cox models estimate the hazard rate of conflict termination at month t for a given conflict id (i) as a function of a baseline hazard of civil war termination ($h_o(t)$) and our anticipated covariates ($\boldsymbol{x}_i^{'}\beta$). Following Cunningham, Gleditsch, and Salehyan (2009), we employ Breslow's (1974) approximation method to handle tied events in all main analyses, and illustrate robustness to this decision in the online supplementary appendix. We withhold discussion of our Cox models' proportionality assumptions until further below, as this relates to our second hypothesis.

Independent Variable

For each of the state-rebel dyads in our sample, our independent variable is a binary indicator of whether that dyad's state military actor primarily used *conscription* to recruit soldiers (=1) as opposed to employing a primarily voluntary force (=0).[6] By "conscription," we refer to a state's use of force as the principal means by which it inducts individuals into the military, whether through legal means (e.g., a formal draft), extralegal means (e.g., impressment), or other methods of recruitment in which "individuals cannot realistically say 'no' to military service" (Toronto 2014, 3). This variable originally had global coverage for the 1816–2000 time period, for which Asal, Conrad, and Toronto (2017) had extended the Military Recruitment Data Set (Toronto 2014). For our analysis, we update *conscription*'s codings through 2003, so as to match our temporal coverage on *civil war duration*.

Approximately 55 percent of the state military observations in our sample employ conscription for our period of analysis. The vast majority of these states had established their recruitment strategies *prior* to the onset of fighting. Specifically, only 8 percent (9 percent) of all conflicts saw a government choose to adopt (phase out) conscription during an ongoing civil war. We list all post-conflict conscription adopters in table A.15 of the online supplementary appendix. We additionally account for post-conflict conscription adopters in our robustness section to ensure that these atypical cases are not driving our results for strategic reasons or otherwise.

[6] Most military personnel systems include both a professional volunteer component and a conscripted service component. Hence, the relevant units of comparison are not all-volunteer systems versus all-conscript systems, but rather military personnel systems that are predominately volunteer versus predominately conscripted.

Our second hypothesis posits that the conflict-prolonging effects of *conscription* on *civil war duration* will intensify over the course of a civil war. To evaluate this, we follow Box-Steffensmeier, Reiter, and Zorn (2003) and create an interaction between our annual *conscription* measure and each corresponding conflict's logged number of civil war days elapsed at each annual time point (*conscriptXlnT*). In our tests of H2, we include *conscriptXlnT* in our model alongside *conscription*. In the context of Cox models, and unlike standard interaction model tests, including these two terms (but not *lnT* itself) relaxes the proportionality assumption of the Cox model and allows one to evaluate the potential nonlinear effects of a given variable (in our case *conscription*) with respect to time (Box-Steffensmeier, Reiter, and Zorn 2003). To provide the most comprehensive test of H2, these models also include interactions with time for any additional control variables that are found to violate our Cox models' proportional hazards assumptions based upon Schoenfeld residuals tests. We show robustness when using the full range of alternative transformations of time considered in similar past assessments (Box-Steffensmeier, Reiter, and Zorn 2003), and their interactions with *conscription*, in table A.14 of the online supplementary appendix.

Model Specifications and Control Variables

Our assessments of the effects of *conscription* (and *conscriptXlnT*) on *civil war duration* precisely mirror the Cox model specifications used for *civil war duration* within Cunningham, Gleditsch, and Salehyan (2009). We then extend these specifications in our main analyses as well as within our robustness section and online supplementary appendix. In this subsection, we primarily describe the model specifications and control variables used in our assessment of H1. The extensions to these specifications that are used to evaluate H2 are presented further below, after fully evaluating our primary findings with respect to H1.

Given that Cunningham, Gleditsch, and Salehyan (2009) provide one of the most comprehensive assessments of the effects of relative rebel and government fighting capabilities on civil war duration to date, we first replicate both of the primary model specifications that they report. Model 1 includes a full set of disaggregated measures of rebel-fighting capabilities drawn from Cunningham, Gleditsch, and Salehyan (2009), including binary indicators for whether (=1) or not (=0) the rebel group actor within a particular dyad exhibited a *strong central command*, a *high mobilization capacity*, a *high arms-procurement capacity*, or a *legal political wing*. Model 2 instead includes a pair of binary indicators of relative rebel strength. These two indicators specifically denote whether a dyad's rebel group is identified as being (1) stronger than state military forces (*rebels stronger*) or (2) at parity with state military forces (*rebels at parity*), with a third (omitted) baseline category denoting instances where rebels are weaker than state military forces.

Model 3 then reports a combined specification that includes all variables from both of Cunningham, Gleditsch, and Salehyan's aforementioned specifications alongside a control variable that counts the total number of *prior* (internal dyadic) *conflicts* (1945–2003) that had concluded prior to a given conflict dyad-year for each state actor in our data.

All of the model specifications outlined above also control for additional, civil war–specific binary indicators denoting whether a given conflict saw rebels hold a degree of *territorial control*, could be considered a *war on core territory*,[7] was

[7] To distinguish between these and wars fought within colonies.

initiated as a *coup d'etat*, occurred within a country experiencing *two or more* (active civil war) *dyads*, or was considered to be an *ethnic conflict*.[8] In addition, they include country-year level controls for ethnolinguistic fractionalization (*ELF index*), the natural log of gross domestic product per capita (*ln GDP per capita*), *democracy*,[9] and *ln population*.

In tables A.8–A.11 of the online supplementary appendix, we also report versions of *each* model specification outlined here when an expanded set of controls are included. We first seek to hold constant a country's time-varying civilian employment prospects via a control for *GDP growth*. Next, in light of existing findings that *competitive intervention* prolongs civil wars, we control for whether (=1) or not (=0) both government and rebel forces received external support from different third-party states in the same conflict month (Anderson 2019).[10] We then include Cunningham, Gleditsch, and Salehyan's indicator for *prior dyadic conflict* and add a control for the logged total number of state *military personnel* (Singer, Bremer, and Stuckey 1972). We then include all of Asal, Conrad, and Toronto's identified country-year predictors of *conscription* so as to additionally control for a state's annual *military expenditure/GDP*, *Polity* score (Marshall, Jaggers, and Gurr 2010), and *energy consumption per capita* (Singer, Bremer, and Stuckey 1972), as well as a binary indicator for the existence of an interstate *rivalry* (Thompson 2001) and a binary indicator for whether (=1) or not (=0) a state year also saw *international conflict* (Sarkees 2000). Finally, we add a series of controls to further account for state military labor market demand and functionality: *political corruption* (Coppedge et al. 2021), *ln military expenditure/military personnel*, and *military personnel/population* (Singer, Bremer, and Stuckey 1972). Our robustness models then separately consider controls for *forced rebel recruitment* (Cohen 2013), conflict type (Cunningham, Gleditsch, and Salehyan 2009), and the presence of *pro-government militias* (PGMs) (Carey, Mitchell, and Lowe 2013). Summary statistics for all variables appear in table A.1 of the online supplementary appendix.

Results

We first discuss our results for H1, before turning to H2. The Cox model results corresponding to the former assessment appear in models 1–3 of table 1. Importantly, because the coefficient estimates in table 1 report the effects of our covariates on the baseline hazard of a civil war's termination, positive coefficient estimates imply increases in the hazard of conflict termination (i.e., shorter civil war duration) and negative estimates imply decreases in the hazard of conflict termination (i.e., prolonged civil war).

In models 1–3, the coefficient estimates for *conscription* are negative and statistically significant at the $p < 0.05$ level. In support of H1, this implies that the presence of conscription (versus a volunteer force) within a state's military is associated with a decrease in the baseline hazard of civil war termination, and hence a longer civil war. As for the statistically significant control variables in models 1–3, the coefficient estimates for *wars on core territory* and (rebel) *territorial control* are each consistently negative and significant. This is in line with Cunningham, Gleditsch, and Salehyan's findings that non-colonial civil wars, and civil wars with substantial rebel territorial control, last longer than conflicts with less territorial salience. Likewise, *democracy* and *two or more dyads* are negative and statistically significant in models 1–3, implying that democracies and civil wars involving multiple warring dyads each experience prolonged conflict. Also consistent with past research, *rebels stronger*, *rebels at parity*, *ln GDP per capita*, *coup d'etat*, *legal political wing*, and *strong central command* are each at times associated with shorter civil wars.

To evaluate these findings substantively, we use the estimates from model 3 to plot survival curves of civil war duration in the presence and absence of *conscription*, as well as for 0-to-1 changes in three additional control variables that have been broadly evaluated within extant civil war analyses (Cunningham, Gleditsch, and Salehyan 2009; Bagozzi 2016): *strong central command*, *democracy*, and *territorial control*. All survival curves were calculated while holding other variables to their means or modes, and appear in figure 1. Turning to our survival curves for *conscription*, we find that—for any point in time—the predicted likelihood of civil war termination is higher for civil conflicts whose state militaries do not have conscription than for those that do. Thus, civil wars involving state militaries that rely on conscription are longer than civil wars involving state militaries that rely on volunteers. This effect is notable in its substantive size. At our sample's mean level of civil war duration (roughly 4.8 years), instituting conscription is estimated to decrease a conflict's probability of termination from 39 to 24 percent. This −15 percentage point change is larger in size than that of *strong central command* (+9 percent), comparable in size to that of *territorial control* (−18 percent), and half the size of the effect of a full shift from non-*democracy* to *democracy* (−30 percent).

We next assess whether these conflict-prolonging effects of *conscription* intensify over time (H2). We first follow Box-Steffensmeier, Reiter, and Zorn (2003) to examine plots and Schoenfeld residuals tests from models 1–3. Doing so allows us to formally evaluate whether our Cox models' proportional hazards assumptions have been violated, both for our individual covariates and model-wide. We report the results of these Schoenfeld residuals tests in table A.2 of our online supplementary appendix. Therein, we determine that *conscription*, *coup d'etat*, *high fighting capacity*, *ethnic conflict*, and *two or more dyads* generally violate models 1–3's proportional hazards assumptions. The consistently significant ($p < 0.01$) finding for *conscription* in this regard offers preliminary support for H2. To further evaluate this potential, we create interactions between each offending variable and the log of time (*ln time*), and include these interactions within models 4–6 of table 1. Doing so allows us to evaluate whether—and how—the effects of each of these five variables varies (in)consistently over time. In line with best practices for modeling nonproportional hazards in this fashion (e.g., Box-Steffensmeier, Reiter, and Zorn 2003; Licht 2011), we withhold the constitutive term for *ln time* from these models.

These nonproportional hazard assessments are strongly supportive of H2. Note first that the coefficient estimates for *conscription* in models 4–6 no longer match those of models 1–3. Rather, because we have included *conscriptXlnT* in models 4–6, the coefficient estimates for *conscription* now only reflect the effect of conscription on the baseline hazard of civil war termination when $lnT = 0$ (i.e., for the case of a civil war with only one elapsed day). This effect must therefore be interpreted alongside the estimated effect of *conscriptXlnT*. Across models 4–6, *conscription* is positive and statistically significant ($p < 0.01$). This suggests that at the very outset of a civil war, *conscription* reliably exhibits a small positive

[8] Based upon Cunningham, Gleditsch, and Salehyan's own classifications.

[9] Coded 1 for country-years with a Polity IV (Marshall, Jaggers, and Gurr 2010) value ≥ 6.

[10] We construct this indicator variable using data on external support provided in Cunningham, Gleditsch, and Salehyan (2009).

Table 1. Cox estimates of civil war duration, 1945–2003

	No ln time interactions			*Ln time interactions*		
	Model 1	*Model 2*	*Model 3*	*Model 4*	*Model 5*	*Model 6*
Conscription	−0.394**	−0.368**	−0.427**	1.894***	1.860***	1.935***
	(0.164)	(0.172)	(0.172)	(0.476)	(0.515)	(0.482)
ConscriptXlnT				−0.385***	−0.383***	−0.397***
				(0.070)	(0.074)	(0.070)
Territorial control	−0.369**	−0.437***	−0.491***	−0.384***	−0.423***	−0.504***
	(0.143)	(0.153)	(0.151)	(0.147)	(0.158)	(0.159)
Strong central command	0.274*		0.280*	0.308*		0.303*
	(0.163)		(0.158)	(0.158)		(0.161)
High mobilization capacity	0.264		0.225	0.222		0.196
	(0.187)		(0.183)	(0.173)		(0.171)
High arms-procurement capacity	0.426		0.364	0.616		0.574
	(0.456)		(0.415)	(0.521)		(0.488)
High fighting capacity	0.427		0.052	−1.100*		−1.451**
	(0.322)		(0.423)	(0.573)		(0.578)
High fighting capXlnT				0.273***		0.282***
				(0.082)		(0.084)
Legal political wing	0.567***	0.543***	0.570***	0.497***	0.458**	0.498***
	(0.190)	(0.191)	(0.189)	(0.183)	(0.182)	(0.181)
War on core territory	−0.578**	−0.638**	−0.635**	−0.712***	−0.726***	−0.748***
	(0.289)	(0.282)	(0.297)	(0.270)	(0.270)	(0.273)
Coup d'etat	2.031***	2.101***	2.028***	5.414***	5.021***	5.333***
	(0.334)	(0.319)	(0.321)	(0.969)	(0.960)	(0.945)
CoupXlnT				−0.732***	−0.652***	−0.728***
				(0.158)	(0.158)	(0.155)
ELF index	0.325	0.248	0.292	0.542*	0.468	0.495
	(0.302)	(0.297)	(0.302)	(0.313)	(0.310)	(0.317)
Ethnic conflict	−0.099	−0.043	−0.035	4.824***	4.755***	4.744***
	(0.185)	(0.179)	(0.183)	(0.882)	(0.859)	(0.868)
EthnicXlnT				−0.746***	−0.731***	−0.728***
				(0.124)	(0.122)	(0.124)
Ln GDP per capita	0.134	0.154*	0.135	0.156*	0.182**	0.158*
	(0.084)	(0.082)	(0.085)	(0.083)	(0.083)	(0.086)
Democracy	−0.823***	−0.853***	−0.850***	−0.724***	−0.755***	−0.749***
	(0.196)	(0.204)	(0.201)	(0.188)	(0.197)	(0.196)
Two or more dyads	−0.454***	−0.436***	−0.421***	0.607	0.423	0.596
	(0.123)	(0.127)	(0.127)	(0.579)	(0.570)	(0.582)
DyadsXlnT				−0.154*	−0.124	−0.147*
				(0.084)	(0.082)	(0.084)
Ln population	−0.065	−0.035	−0.021	−0.093*	−0.079	−0.049
	(0.056)	(0.056)	(0.063)	(0.055)	(0.056)	(0.066)
Rebels stronger		0.836***	0.532		0.322	0.413
		(0.251)	(0.362)		(0.270)	(0.427)
Rebels at parity		0.631***	0.580***		0.543***	0.563***
		(0.182)	(0.181)		(0.194)	(0.197)
Prior conflicts			−0.028			−0.027
			(0.031)			(0.032)
Number of conflicts	436	436	436	436	436	436
Number of failures	348	348	348	348	348	348
Observations	2,415	2,415	2,415	2,415	2,415	2,415

Notes: Standard errors clustered on conflict ID are given in parentheses; ln corresponds to natural log.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

effect on the likelihood of immediate war termination. However, the negative and statistically significant ($p < 0.01$) coefficient estimate on the interaction between *conscription* and the log of time (i.e., *conscriptXlnT*) in models 4–6 suggests that this positive effect is quickly overridden by a negative (and thus conflict-prolonging) effect over time, and increasingly so. Hence, not only does conscription have a conflict-prolonging effect overall (H1), but its conflict-prolonging effect increases in its impact as a civil war progresses (H2).

To fully evaluate this latter effect, we follow Licht's (2011) recommendations for evaluating covariate effects in the context of nonproportional hazards by plotting the combined coefficient estimate for *conscription* and *conscriptXlnT* from model 6. This plotted quantity is found within the bold line in figure 2, with 95 percent confidence intervals appearing in the corresponding thin lines. The gray dashed line depicts *civil war duration*'s sample distribution; the vertical line that appears at the approximate 300 day mark denotes

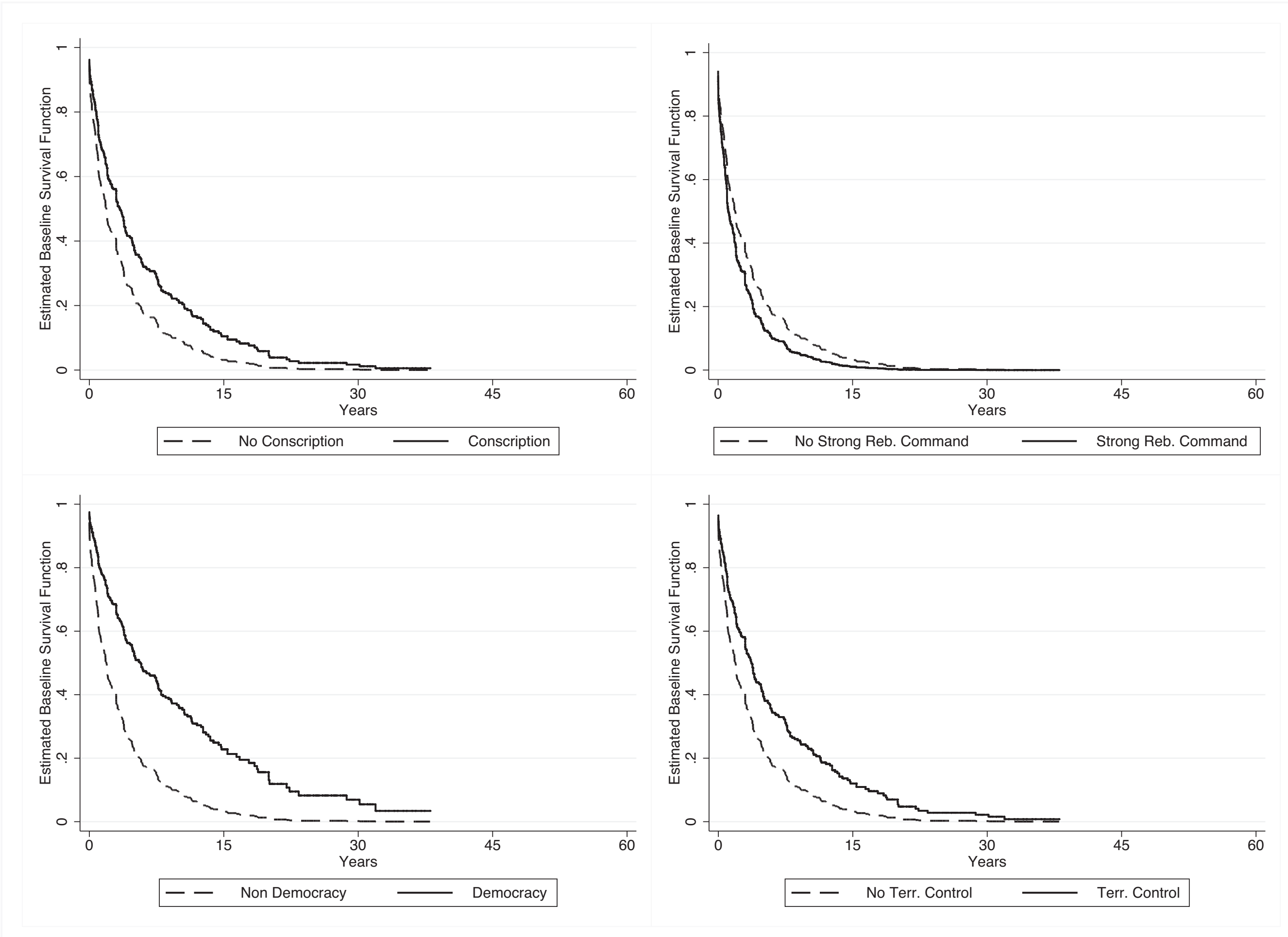

Figure 1. Comparisons of conditional survival rates for civil war.

the point at which our combined coefficient estimate becomes statistically significant. We find that the effect of *conscription* is at first positive but quickly becomes negative after 130 days of fighting. This negative, conflict-prolonging effect becomes significant at the 306 day mark, remaining significant—and continuing to grow—thereafter. The average conflict duration in our sample is 4.8 years and over 70 percent of the civil wars in our sample endure beyond the 306 day mark, confirming that our statistically significant, combined negative estimate for *conscription* encompasses a majority share of the conflicts in our data. Hence, and in line with H2, the conflict-prolonging effects of military conscription substantially increase in magnitude as civil wars endure.

Robustness Assessments

Our results are robust to a wide range of alternate model specifications. In light of space constraints, we report the robustness assessments discussed below in tables A.3–A.14 of the online supplementary appendix.

Given extant theoretical contentions that democracies prefer capitalized militaries (Caverley 2014) and that democracy conditions the effects of military conscription on interstate conflict (Vasquez 2005), one possibility is that our findings are primarily driven by the democracy cases in our sample. To evaluate this, we separately restrict our sample cases to only democracies and to only non-democracies.[11] Our conclusions for H1 and H2 remain for our non-democracy subsample; those for H2 remain for our (significantly reduced) democracy-only subsample. These results confirm that the conclusions above are not driven by the relatively small number of democracy cases in our sample. They also add nuance to existing work on regime type, war finance, and war duration. Caverley (2014), for example, argues that democracies prefer heavily capitalized militaries and that capitalization increases duration in small wars. We find that, in the context of intrastate conflict, labor-intensive conscript militaries fight longer wars, and that this effect holds regardless of regime type.

We next return to our full sample and omit all Israeli civil conflicts. Our findings are not sensitive to our exclusion of the Israel–Palestine conflict, which is a relative outlier in terms of conflict duration, the number of conflict dyads, and the use of conscription. We also demonstrate that our conclusions with respect to *conscription* (and *conscriptXlnT*) remain when all specifications in table 1 are reestimated in a manner that addresses event ties via Efron's method as opposed to Breslow's method. Following this, we illustrate that our findings for H1 and H2 continue to hold after omitting the roughly 8 percent of civil wars in our sample that saw militaries adopt conscription after the start of a civil war. We then demonstrate that our findings for *conscription* (and

[11] Based upon *democracy*, which was coded 1 for country-years with a Polity IV value ≥ 6. We must omit our *coup*-based variables from two democracy specifications to achieve model convergence.

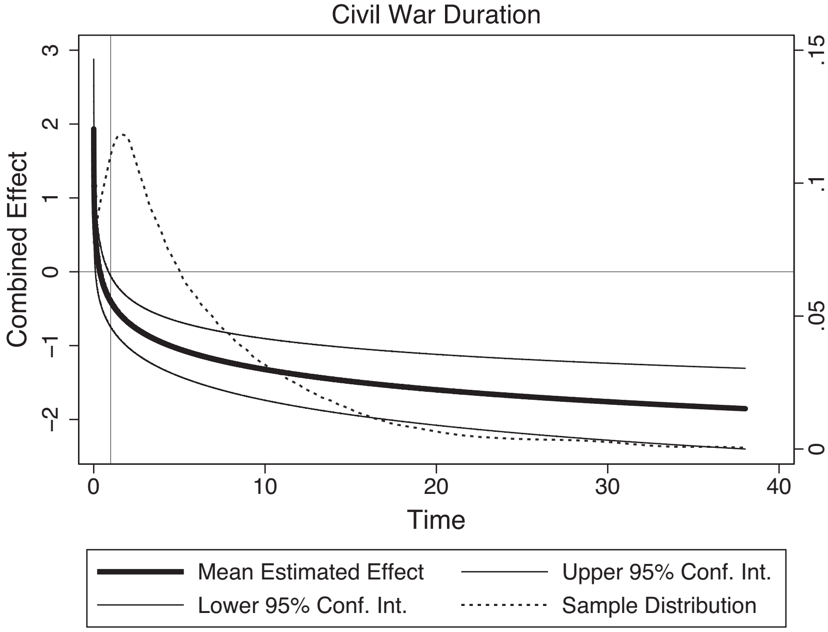

Figure 2. Combined coefficient effect for *conscription* and *conscriptXlnT*.

conscriptXlnT) are also robust to the inclusion of additional correlates of *civil war duration* and/or *conscription*, including *GDP growth*, *competitive intervention*, *international conflict*, *military expenditure*, measures of international *rivalry*, a full *polity* scale, an indicator for whether a dyad was previously active in conflict, the government's *ln military personnel*, *energy per capita*, *political corruption*, *ln military expenditure/military personnel*, and *military personnel/population*.

Next, we account for variation in rebel recruitment strategies via Cohen's (2013) *forced rebel recruitment* measure. This measure is only available from 1980 onward and for only a subset of our sample's post-1980 civil wars. As such, including *forced rebel recruitment* in our models listwise deletes 49 percent of our observations and limits the control variables that we can include due to an absence of (temporal) variation in this subsample. These issues notwithstanding, our core findings remain across all six model specifications when controlling for *forced rebel recruitment*. Likewise, the subsequent models in the online supplementary appendix demonstrate our results' robustness to eight additional dichotomous controls for conflict type, coded based upon the dyadic conflict-type designations in Cunningham, Gleditsch, and Salehyan (2009).

Next, we add an additional control for the presence of PGMs. States may at times compensate for the cost of military labor by relying on PGMs (Carey, Mitchell, and Lowe 2013). In most cases, PGMs support military activities in particular areas and domains rather than serving as the main fighting force. Nevertheless, we reevaluate our findings when controlling for the presence of (semiofficial or informal) PGMs in a country. This control is only available for a portion of our full sample, and including it forces us to omit (1) a substantial share of our observations and (2) *war on core territory* due to a lack of variation in our retained cases. Our findings for H1 point in the anticipated direction, although are no longer statistically significant. However, we continue to find support for H2, even after controlling for *PGM*.

Our next set of robustness models reevaluate our primary results on a preprocessed (matched) sample. While conscription decisions were typically made prior to the civil wars considered in our analysis, there remains the potential for endogenous conscription adoption. If this is the case, then imbalance in observables across our *conscription* and non-*conscription* cases may be influencing our findings. Matching provides one means for addressing this issue. To implement matching in our time-varying survival data context, we follow the strategy outlined in Arias, Hollyer, and Rosendorff (2018). This entails that we first collapse our data and match[12] on cross-sectional (in our case, conflict dyad) averages of selected *conscription* predictors.[13] These cross-sectional averages correspond to mean covariate values across all observed *t*'s for our censored conflict dyads and to mean covariate values across all *t*'s prior to the *t* of war termination for our terminated conflict dyads. After matching, we decompress our data back to our original unit of analysis and reestimate our survival models for only our matched cases with standard errors clustered by matching pair membership. The results from this post-matched sample remain supportive of our key findings.[14]

Next, recall that like Cunningham, Gleditsch, and Salehyan (2009), our survival data include annually time-varying covariates but subannual variation on *civil war duration*. When addressing nonproportional hazards in Cox model contexts, Jin and Boehmke (2017) suggest expanding one's time intervals to more precisely match the scale of one's analysis time by splitting one's spells at all observed failure times. Doing so in our case increases our *N* by a factor of 20 and drastically increases our event ties.[15] Yet, as the online supplementary appendix demonstrates, our key

[12] We employ genetic matching (Diamond and Sekhon 2013) without replacement with a caliper of 0.1 standard deviations. This retains forty-four *conscription* and forty-four non-*conscription* dyads, and yields improved and sufficient balance with a majority of covariates falling at or within standard thresholds (see figure A.1 in the online supplementary appendix).

[13] Specifically, the correlates of *conscription* identified in Asal, Conrad, and Toronto (2017) alongside the remaining country-year predictors from table 1 (i.e., *ln population*, *ELF index*, and *ln GDP per capita*).

[14] Because our matched sample is considerably smaller than our primary sample, we lose variation on *coup d'etat* and must omit it (and/or *coupXlnT*) in some specifications.

[15] Leading us to favor Efron's method for handling ties in this case.

findings with respect to H1 and H2 continue to hold under this alternative formulation.

Finally, table A.14 reestimates our nonproportional hazards models for H2 when alternately operationalizing time (and its interactions with *conscription*, *coup d'etat*, *ethnic conflict*, and *two or more dyads*) in three additional manners proposed by Box-Steffensmeier, Reiter, and Zorn (2003). In each case, our results for *conscription* and its interaction with time remain statistically significant in the directions discussed above.

Taken together, the results reported here strongly support this article's theoretical claims. State military conscription prolongs civil wars—an effect that is reinforced as a conflict drags on. This is a generalizable finding: one that holds across the wide range of civil war contexts around the globe. It is also a robust result, which holds across a wide range of modeling choices and alternate specifications.

Conscription in Civil War: Illustrative Cases

We briefly explore two cases, drawn from our larger sample, to validate the mechanisms linking conscription to prolonged civil war. We select our cases using two case selection approaches. We select our first case using the "typical" selection approach advocated by Seawright and Gerring (2008, 299), identifying a civil war where conscription existed throughout the conflict, thereby holding our first explanatory variable constant. This allows us to identify how conscription shaped variation in civil war duration over the entire conflict's span. We choose Colombia because it fulfills these conditions. The country fought a long civil war against the Fuerzas Armadas Revolucionarias de Colombia (FARC) between the 1960s and 2010s. A compulsory draft existed in Colombia throughout the entire conflict, backed by punishments and even military raids to abduct young men (Matallana-Villarreal 2020). For our second case, we employ Seawright and Gerring's (2008, 300) "diverse" case selection approach, where we identify variation in both conscription and conflict duration. Here, our focus is on the Sandanistas' 1981–1989 war against the Contras in Nicaragua. As the online supplementary appendix indicates, this case represents one of the few instances of a government adopting conscription after the onset of a civil war. It thereby provides an opportunity to evaluate the impact of "switching" into conscription in the midst of fighting.

Colombia's War against the FARC

Colombia's war against the FARC illustrates not only how conscription can prolong civil wars, but also the viability of our underlying mechanisms. It has been estimated that conscripted soldiers in the Colombian forces earned one-tenth the wage of volunteers (Priestley 2000, 5). In 2003, the average soldier made just $175 per month (Adams 2003). As former Defense Minister Marta Lucia Ramirez acknowledged, "Colombian soldiers are very poorly paid. This is the sad reality. We have to be aware that we have a group of men and women who every day are risking their lives for Colombia and really earning very little" (quoted in Adams 2003). And yet, despite their paltry compensation, it was conscripts that made up the bulk of Colombia's military labor in the fight against the FARC.

In line with our proposed *bargaining range effect* mechanism, case evidence suggests that the availability of cheap, conscript labor reduced the incentive of the Colombian government to compromise. While there were several attempts at negotiated settlement during the war, Colombia's political leaders regularly failed to offer the concessions that the FARC demanded. The ruling elite demonstrated a "lack of concern" about the insurgency (Marks 2002, 4), while military leaders consistently overestimated their chances of victory. As General Charles Wilhelm, former chief of the US Southern Command explained, "[t]he primary vulnerability of the Colombian armed forces [was] their inability to see threats, followed closely by their lack of competence in assessing and engaging them" (quoted in Farah 1998). Even amidst a nation-wide guerrilla offensive in 1998, the then head of Colombia's armed forces, General Fernando Tapias, insisted that "[t]he military situation is improving [...] the army is not losing the war" (quoted in Ruiz 2001, 20). Happy to leave the direction of the counterinsurgency campaign to the military, Colombia's political leaders demonstrated little interest in serious bargaining. As Marks (2002, 11, emphasis in original) put it, "the essential counterinsurgency problem" was that "the *country* [was] not engaged in fighting its own internal war. The business [was] left to the military."

Yet, the Colombian military proved unable to decisively confront the numerically inferior FARC. In keeping with our proposed *capital–labor substitution effect* mechanism, there is clear evidence that at least part of the military's lack of success was attributable to an over-reliance on manpower and insufficient investment in human and physical capital. The armed forces enjoyed an "ample supply" of infantry weapons and ammunition, but suffered "severe" shortages in crew-served weapons, communications gear, trucks, and helicopters (Marks 2002, 13). Even as late as the mid-1990s, the FARC was better equipped and better trained than the military. As one reporter explained, "[s]tretched thin, filled with teenage conscripts, and outfought by rebels who are more experienced, the army is struggling just to keep insurgents at a military stalemate [...] In contrast, many of the 12,000 fighters in the FARC have lengthy combat experience, receive monthly stipends of up to about $400, and wield better radio equipment and rifles" (Johnson 1997). Facing equipment shortfalls and relying on poorly trained conscripts, the military was unable to sufficiently integrate its limited air power capacity with its ground forces, enabling the FARC to evade government forces in remote, rural areas (Caverley and Sechser 2017, 707). And while the Colombian armed forces' national headquarters were integrated, "this did not extend beyond the building," severely hampering combined arms operations (Marks 2002, 13). Even as violence flared in the early 2000s, the majority of Colombia's armed forces were "protecting fixed targets" (Sweig 2002, 135), "tied down to static defense duty" (Marcella 2001, 18), or "dedicated to point defense" (Marks 2002, 10)—a force employment strategy illustrative of the capital–labor distortions typical of conscript militaries.

The Colombian state's unwillingness to compromise, together with its inability to bring the fight to the FARC, delayed a negotiated settlement to the war. This aligns with both the bargaining range effect and capital–labor substitution effect mechanisms that link conscript personnel systems to protracted civil wars.

Nicaragua's War against the Contras

Following their rise to power in 1979 and the subsequent onset of the Contra counterrevolution in 1981, the Sandinista government of Nicaragua initially had little trouble with voluntary military recruitment (Department of State 1985). However, public disillusionment with the Sandinistas—alongside pressures to defeat an increasingly well-armed and highly financed insurgency—quickly led the government to propose and then institute a draft beginning in 1984.

By 1985, the draft had already "helped turn Nicaragua's army into Central America's largest" (Williams 1985a). In line with our hypothesized *bargaining range effect* mechanism, this appears to have decreased the Sandinistas' openness to ending the conflict through dialogue or deescalation. For example, and again in 1985, the Sandinista military began the largest anti-Contra offensive it had launched to date: "Fortified with newly diversified troop units and a willingness to risk conflicts with neighboring countries," the Sandanista government found itself "in the midst of an all-out effort to seal off the infiltration routes of anti-Sandinista rebels" (Williams 1985b). The following year, the government likewise banned the Nicaraguan media from even mentioning appeals for negotiations from religious leaders (Boudreaux 1985).

A number of additional attributes of Nicaragua's use of conscription—and of the Sandinistas' war against the Contras—are also in line with our *capital–labor substitution effect* mechanism regarding under-trained manpower in conscript militaries. For instance, the US State Department (1985, 35) noted that the onset of Sandinista conscription in 1984 "led to broad resentment over the inadequate training given to SMP [Servicio Militar Patriótico] recruits. Often draftees without adequate military skills are sent to the front to face the forces of the armed opposition. Increasingly, many Nicaraguan parents believe that the Sandinistas are using their children for cannon fodder." As the use of conscription entered into effect and the war dragged on, our broader capital–labor substitution implications regarding underequipped manpower also took hold, with the Nicaraguan military increasingly being characterized in manners such as "a haphazardly organized and equipped Sandinista armed force that is short of not only weapons and ammunition but also basics like food, clothing and medicine" (Holloran 1987).

At least partly as a result, the Contras continued to undertake military offensives and achieved a number of key military gains against the Sandinistas, most notably in southern Nicaragua in 1987 (Lemoyne 1987). This prolonged a conflict that many expected to have ended by that point in time, including Sandinista leader Daniel Ortega (Kinzer 1987). These observations again directly align with our bargaining range effect mechanism in their highlighting the Nicaraguan government's inability to correctly estimate the Contras' resolve and capabilities.

Three years after the initiation of conscription in Nicaragua, both sides were still resistant to ending the conflict. With peace talks gaining momentum both internally and externally in 1987, journalists observed that "the Sandinistas—like the Contras—show no signs of stopping the recruitment that has swelled their Army from 24,000 in 1981 to over 100,000 today, including more than 50,000 reserves" (Larmer 1987). Another 2 years would pass before the conflict was ultimately concluded in 1989.

Conclusions and Implications

Because states with volunteer militaries must pay higher wages to attract recruits from the wider labor market, their budgetary costs of war are higher relative to conscript militaries. As war becomes more costly in this manner, the set of agreements the government prefers to war expands, the production of military power becomes more capital intensive, and the hazard of conflict termination rises. In contrast, states with conscript militaries can pay lower wages, given that they do not compete to attract recruits from the wider labor market. As war becomes less costly in this manner, the set of agreements the government prefers to war expands more slowly, the production of military power becomes more labor intensive, and the hazard of conflict termination declines. We demonstrate that these *bargaining range effects* and *capital–labor substitution effects* are reinforced as a civil war drags on, leading to an increased divergence in conflict duration across conscripted and volunteer militaries over time.

These arguments resonate with a wider set of empirical findings on the relationship between military personnel systems and war. For example, an additional observable implication of our bargaining range effect mechanism is the expectation that conscript militaries are more likely to see combat relative to volunteer militaries, given conscript militaries' lower expected costs of war. Pickering (2011) provides evidence in support of this expectation. Similarly, our capital–labor substitution effect mechanism would anticipate that volunteer militaries take on fewer casualties than conscript militaries, given that volunteer militaries' production of military power is likely to be more capital, and less labor, intensive. Horowitz, Simpson, and Stam (2011) report evidence verifying this prediction.

Our results also speak to existing work on the politics of war finance. Kreps (2018), for example, shows that US leaders have sought to avoid angering the American public by financing wars through borrowing, rather than taxation. This approach defers the costs of war, enabling politicians to sidestep the economic consequences of lengthy conflicts. In our account, conscription plays a related role: by reducing the budgetary costs of war in the short term, it enables states to avoid fiscal tax increases. This provides political space for state leaders to drive harder bargains, even when confronting protracted rebellions.

Our findings also stand to inform important policy debates about the utility of conscription in the production of military power. Proponents of conscription highlight the high budgetary costs of volunteer personnel systems and the need for government access to large labor pools in the face of internal and external threats. Yet, while it is true that conscription enables states to reduce their annual budgetary costs of war by exploiting involuntary labor, our findings suggest that these short-term fiscal benefits must be weighed by countervailing long-term consequences—among which include an increased likelihood of prolonged military campaigns. While the yearly wage bill will generally be lower under a conscript system, the over-time budgetary savings of conscription will be illusory if a conflict's duration is extended. This provides another, heretofore overlooked, economic argument against conscript military systems.

Finally, the results reported above contribute to a growing body of scholarship that highlights how technologies of war determine the conduct of armed conflict (e.g., Biddle 2004; Kalyvas and Balcells 2010; Caverley and Sechser 2017). Existing work has shed light on the ways in which mechanization, strategy, and relative strength shape and constrain combatants' use of force. Yet to date, the significance of a state's military personnel system has been overlooked in the intrastate conflict literature. This article addresses this lacuna by demonstrating the importance of incorporating state recruitment strategies into theoretical and empirical models of civil war. In doing so, it highlights the potential contributions of future research on the labor-side determinants of civil war outcomes. Future research should also explore the potential for interactive effects between the labor policies of state militaries and different technologies of rebellion in the dynamics and duration of conflict (Kalyvas and Balcells 2010). As our results can attest, states' capacities to recruit and retain capable individuals for military service play a critical role in their willingness and ability to keep fighting.

Supplementary Information

Supplementary information is available in the *International Studies Quarterly* data archive.

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